privé de ses poussières flottantes. Dès lors je dois appliquer privé de ses poussières flottantes. Dès lors je dois appliquer au docteur Bastian ces paroles de mon mémoire de 1862, pages 70 et 71: "En présence de ces résultats (résultats que je viens de rappeler et qu'accepte le docteur Bastian), un partisan de la génération spontanée veut-il continuer à soutenir ses opinions? Il le peut encore; mais alors son raisonnement sera forcément celui-ci: 'Il ya dans l'air, dira-t-il, des particules solides, telles que carbonate de chaux, silice, suie, brins de laine, de coton, fécule . . . et à côté, des corpuscules organisés d'une parfaite ressemblance avec les spores des mucidinées ou avec les kystes ressemblance avec les spores des mucidinées ou avec les kystes des infusoires. Eh bien, je présère placer l'origine des mucidinées et des infusoires dans les premiers de ces corpuscles, ceux qui sont amorphes, plutôt que dans les seconds.'" L'inconsé-quence d'un pareil raisonnement ressort d'elle-même et le progrès de mes recherches consiste à y avoir acculé les partisans de l'hétérogénie. Lisez attentivement l'article précité du docteur Bastian et vous verrez qu'il se résume en effet, dans le raisonnement que je viens de reproduire. Le docteur Bastian et vous de l'article précité du docteur Bastian et vous verrez qu'il se résume en effet, dans le raisonnement que je viens de reproduire. Le docteur Bastian de placer de la constant de le produire de la constant de le produire de la constant de le produire de la constant de la consta me permettra de placer dans sa bouche ces paroles:--"C'est bien vrai, les expériences de M. Pasteur et celles de M. Tyndall m'ont acculé, moi Docteur Bastian, partisan de la génération spontanée, dans cette déclaration. Oui, je préfère recourir sans motif sérieux, à la croyance à une force résidant, dans la partie amorphe des poussières en suspension dans l'air plutôt que de la placer cette force dans la partie organisée formée de corpuscules identiques d'aspect à ceux des germes des organismes des infusions." Parler ainsi n'est-ce pas avouer sa

Quelles sont donc ces particules amorphes dont vous invoquez si gratuitement l'influence et de quel droit leur attribuez-vous les primum movens de la vie? Pourquoi, si vous aviez raison, ne le trouverait-on pas ce primum movens dans les particules amorphes ou organisées qui existent à l'etat naturel dans le sang frais, dans l'urine fraîche, dans le jus du raisin, quand on expose ces liquides dans l'air pur? Voulez-vous que vos particules amorphes, douées du primum movens de la vie des infusions, sortent de matières déja alterées, putrides, etc. . . . mais, pourquoi seraient-elles charices par l'air sans être accompagnées des germes et des êtres vivants de ces infusions et, s'il en est ainsi, comment ne pas placer le primum movens de la vie dans ce qui est vivant, plutôt que dans ce qui n'a rien des caractères apparents de la vie?

Elle est inattaquable, cette conclusion que j'ai déja formulée: dans l'état actuel de la science, l'hypothèse de la génération spon-tanée est une chimère.

Votre bien dévoué,
Paris le 8 Fevrier, 1876

L. PASTEUR

Mr. Sorby on the Evolution of Hæmoglobin

In the short notice in NATURE (vol. xiii. p. 257) of my paper on the Evolution of Hæmoglobin, in the Quarterly Microscopical Yournal, it is said that my conclusions are mainly based on a small difference in the wave-length of the absorption-bands of the spectrum of the red blood of *Planorbis*. This is, however, a very small part of the question. The principal results are that hæmatin is first met with in the bile of many pulmoniferous molluscs in an abnormal state, quite unfit to serve the purposes of respiration, but easily changed into the normal, which could, and probably does in some cases, perform that function. Then in the blood of *Planorbis* we have a *solution* of a hæmoglobin, in which the hæmatin is combined with an albuminous constituent coagulating at the low temperature of 45° C., and finally we come to the normal hæmoglobin existing as red corpscules, containing an entirely different albuminous constituent, coagulated at about 65° C. In all these changes in the condition of the same fundamental radical, the oxygen carrier becomes of more and more unstable character, and more fitted for the purposes of respiration, as we advance from lower to higher types, as though advantage had been taken of every improvement due to modified chemical or physical constitution. H. C. SORBY

The Flame of Common Salt

In answer to a question put by one of your correspondents (p. 287), allow me to state that the origin of the blue flame in question is still involved in mystery. Your correspondent will find everything that is known on the subject in a letter addressed to the editor of the *Philosophical Magazine*, by Prof. J. H. Gladstone

(Phil. Mag. 1862, vol. xxiv. p. 417).

Prof. Schorlemmer and I are at present engaged in a joint investigation, which we hope will throw some light on the origin of the flame. We have already obtained interesting results, and observed the flame under circumstances in which it has not been seen before, but we are as yet entirely unable to say what the flame is really due to. ARTHUR SCHUSTER

Owens College, Manchester, Feb. 12

Science at Hastings

HAD we here a few more men like Mr. Alex. E. Murray, my paper on "Science at Hastings" would never have been written. But I fail to see in what way he has "vindicated the honour of Hastings." With the exception of one or two sentences which require qualification, his letter is simply an emphatic repetition of what I said in the Hastings and St. Leonards *News*. The substance of my paper may be given in one of its sentences: "With the exception of occasional debates among the members of the Philosophical Society and the few scientific lectures in the winter programme of the Mechanics' Institution, there is in Hastings no public encouragement or aid to science."
As to the Philosophical Society, Mr. Murray admits that, "owing to a variety of circumstances, it is not at present quite so flourishing as we could wish." In point of fact, during the session 1874-75, four papers were read and a conversacione held. This Society is the only distinctively scientific one in the town, notwithstanding the "multiplicity" of institutions mentioned by Mr. Murray. Popular scientific lectures are occasionally given in connection with various associations for young men; and the Mechanics' Institution also has a winter lecture session, but unfortunately the Committee find it very difficult to obtain lecturers, and are fain to eke out their list with musical evenings and readings. The Literary and Scientific Institution has for many years dropped the word "Scientific" from its name, and at present seeks merely to provide for a few of the older inhabitation of the control of the seem of the tants of the town a quiet reading-room supplied with papers, a few reviews and magazines, and a box from Mudie's. Scarcely a new book has been bought for very many years. The meteorological instruments which the Institution "formerly possessed" were, with the exception of the barometer, broken long ago, and the barometer has since been sold. At one time—twenty years ago—I was in the habit of taking the observations in the absence of the gentleman whose special business it was; but it must be at least a dozen years since any observations were systematically taken.

In conclusion, I claim to have fully recognised in my paper all that is being done in Hastings in the interests of science, and I sincerely regret that Mr. Murray has not been able to discover any omission on my part. We have no museum, we have no public library in which there are scientific books recent or numerous enough to be of any use to a student, except in a school or two; we have no Naturalists' or Field Clubs; with the excep-tion of the Philosophical Society, all the existing institutions in Hastings have practically lost what scientific character they may at one time have possessed; and the Philosophical Society itself is neither exclusively scientific nor exclusively local in its aims, and is unfortunately "not quite so flourishing" as could be wished. ARTHUR RANSOM

Hastings, Feb. 5

OUR ASTRONOMICAL COLUMN

THE VARIABLE STAR R LEPORIS.—This highlycoloured star, the variability of which was detected by Schmidt in 1855, is calculated to be at a maximum on the 28th of the present month. The mean period appears to be about 438 days, 230 days being occupied in passing from minimum to maximum, and 208 days from maximum to minimum. Probably the irregularities of variation which have been suspected are to be mainly attributed to the difficulty attending comparisons of a star of such intensely red colour. With regard to the colour, however, there is something more than a suspicion that it has sensibly diminished in intensity since attention was first directed to it (Hind, 1845, October). We are almost wholly indebted to Schmidt, who makes such excellent use of the favourable astronomical conditions under which he is placed at Athens, for our knowledge of the law of variation in R Leporis.

SATURN'S RINGS.—In the last (January) number of the "Monthly Notices of the Royal Astronomical Society" are reproduced some old drawings of Saturn, given in the edition of Gassendi's works published at Lyons, in six volumes, in 1658 (Lalande, Bibliographie Astronomique, p. 245).

Astronomique, p. 245).

In the volume entitled De Annulo Saturni, by E. M. Beima (Augsburg, 1842), a work less known in this country than it deserves to be, will be found other reproductions of the earlier drawings illustrating the appearances which the planet was thought to present in the imperfect telescopes of the time. As a pretty complete monograph up to the date of publication, involving an exposition of the formulæ required in calculating the various phases of the rings, &c., Beima's treatise will be found a very desirable addition to an astronomical library.

The Minor Planet, Hilda (No. 153).—In Herr Kühnert's last orbit of this planet, the aphelion distance is found to be 4595, and the heliocentric latitude in aphelion, — 6° 33′, the longitude at this point being 105° 1′6; hence, the least distance of the planet from the orbit of Jupiter is reduced to 0.564 of the earth's mean distance from the sun. So near an approach might afford an excellent opportunity of determining the value of Jupiter's mass, but if the period of revolution assigned by Kühnert upon eight-weeks' observations is at all approximate, such opportunity will not occur for many years to come. There may be a difficulty in recovering this planet at the next opposition, which is likely to take place near the aphelion, and when its faintness, owing to great distance from the earth, will be considerable; it is the more desirable, therefore, that observations should be obtained in the next period of absence of moonlight, that the mean motion may be fairly determined this season; the Ephemeris published in No. 2,075 of the Astronomische Nachrichten should render the identification of the planet a matter of no great difficulty in instruments of adequate aperture.

From the *resume* of observations in No. 42 of the Circulars of the *Berliner Astronomische Jahrbuch*, it appears that No. 149 may get adrift, unless an observation on Nov. 2 can be proved to belong to it, and No. 155, as already remarked, is in even worse position.

THE TOTAL SOLAR ECLIPSE OF 1706, MAY 11-12.—Calculating upon the same system as employed for the solar eclipses to which reference has already been made in this column, the following elements result for the eclipse of May 1706, extensively observed in France, &c.

Conjunction in R.A. 1706, May	11, 21	h. 59m.	26s. G.M.T.
Ř. A		4	8 40 27
Moon's hourly motion in R.A			36 49
Sun's ,, ,,			2 50
Moon's declination			8 42 52 N.
Sun's		I	8 4 o N.
Moon's hourly motion in decl.			13 9 N.
Sun's ,, ,, ,,			o 36 N.
Sun's ,, ,, ,, ,, ,, ,, ,, ,, ,, ,, ,, ,, ,,			60 3 5
Sun's			9
Moon's true semi-diameter		•••	16 31
Sun's			15 49

Sun's ,, ,, 13 49

The following are points upon the central track of the

iadow :			- ×
Long.	Lat.		Long. Lat.
5° 40′ W.	34° 39′ N.		16° 32′ E. 52° 23′ N.
1 4 E.	40 40		20 2 E. 54 20 N.
4 18	43 29	}	Central at Apparent Noon in Long. 29° 7' E. Lat.
7 37	46 12 N.	{	58° 18′ N.

For examining the circumstances of the eclipse in the South of France, where the totality was witnessed, we have the following reduction equations founded upon a direct calculation for Avignon:—

 $\begin{array}{l} \text{Cos. } w = 41\,\text{1909} - [\text{r'72518}] \sin .\ l + [\text{r'59372}] \cos .\ l, \cos .\ (L - \text{ro3}^{\bullet} 46''3) \\ \ell = 2\text{rh. } 26\text{m. } 5'8\text{s.} + [2\cdot08]38\text{g} \sin .\ w + [3\cdot60351] \sin .\ l \\ - [3\cdot84024] \cos .\ l, \cos .\ (L + 38''\ t'4). \end{array}$

In these equations L is the longitude from Greenwich reckoned positive to the eastward, l the geocentric latitude, and t the Greenwich mean time of beginning or ending of totality, according as the upper or lower sign is used.

PROF. FLOWER'S HUNTERIAN LECTURES ON THE RELATION OF EXTINCT TO EXIST-ING MAMMALIA 1

I.

IF no certain *consensus* has yet been arrived at as to what paleontology teaches in reference to the derivative hypothesis, the chief reason is our very imperfect knowledge of palæontology, arising partly from the necessary imperfection of the geological record caused by the very small chance of the remains of any creature living upon the earth being preserved in a perfect state; partly from the very minute portion of the record which is actually preserved in the rocks having as yet been rendered accessible to investigation; partly from the defective know-ledge of the structure and relationship of those documents, so to speak, which have already been brought to light, and of their existing representatives. The first cause must always remain a stumbling-block to these investigations. The second is gradually being removed by fresh explorations in many parts of the world, notably those now carried on with so much energy and success in North America. The third is one which only needs more numerous and more earnest workers to remove, and especially those who have the power and will to see the continuity of the manifestation of life upon the earth, and will abandon the old practice of studying the fauna of a particular epoch apart from that which preceded or succeeded it, and especially that of studying extinct forms without a thorough mastery of the key to the solution of the difficulties of their structure afforded by the more accessible existing species. Palæontology is no science apart —it can scarcely even be called a branch of zoology; it is simply the application of that science to elucidating the structure of beings now extinct. The thoroughly unscientific and mischievous system of arrangement of nearly all our great public museums, both at home and abroad, where two distinct collections are kept up, under distinct custodians-one for animals existing at the present moment upon the earth, and the other for animals that have existed at all other periods put together-has much to answer for in impeding the progress of sound zoological knowledge, Granted that our information is of a very limited nature, it still seems worth while occasionally to gather together the fragments of which it consists; and as it would be impossible in the time allotted to this course to do justice to more than a limited portion of the whole animal kingdom, it is proposed to take the class of mammals, as in many ways well suited for testing whether such facts as are known of their ancient history throw any light upon their mode of origin, and to point out, with impartiality, the results of the investigation. The poverty of the materials in some quarters, as well as their abundance in others, will thus be made manifest, and some useful land-marks afforded which may direct and stimulate future

As far as we know of the existing fauna of the world, and we can hardly suppose that in this respect our know-ledge is not final, the Mammalia constitute a clearly defined group or class of the Vertebrata. Though covering a wide range of variety in structure, scarcely any zoologist has ever had any hesitation in defining its limits. There are, however, certain forms decidedly aberrant, and which in many of the characters in which they deviate from the ordinary standard of the class, approximate to the lower groups of vertebrates. The most marked examples of

* Abstract of a course of lectures delivered at the Royal College of Surgeons "On the Relation of Extinct to Existing Mammalia, with Special Reference to the Derivative Hypothesis," in conclusion of the course of 1873. (See Reports in Nature for that year.)